already developed in the Alps, a very pronounced longitudinal corrugation came into being during the last phase of the compression of the Alpine chain. As a result of the obstruction offered by the crustal masses in the foreland great culminations and depressions gradually took shape while the flat mountain land was erected into a system of high mountains. Erosion set in and became increasingly active, though it could not keep pace with the uplift or carry away what the folding had built up. In the culmination zones and particularly on the flanks of the chief uplifts fissures were formed and quickly filled with aqueous solutions. These thermal waters had become charged with substances previously dissolved from the surrounding rocks. Crystallization from these solutions set in as the effects of gradually diminishing pressure and temperature made themselves felt. In the course of long crystallization processes crystals of unusual perfection were formed which today are the ornaments of the high Alps. The composition and associations of these minerals give indications as to the temperatures which must have prevailed in the mountain area at the time of their formation. Crystal species which at an earlier period had been formed in the rocks are found to have become unstable and to have undergone unmixing and decomposition, thus giving rise to other minerals. The search for and collection of these crystals not only provide aesthetic pleasure but also furnish much valuable information about the formation of the Alps themselves, of the mountain range, which is the backbone of Switzerland.

BIOLOGY.—*The principle of priority in biological nomenclature.*[1] RICHARD E. BLACKWELDER, U. S. National Museum.

An article under this title by Dr. A. C. Smith of the Arnold Arboretum appeared in Chronica Botanica **9**: 114–119. 1945. It consists largely of a critical review of a paper from the zoological viewpoint by Franz Heikertinger, published in 1942 in Germany[2] which is "an undisguised attack on the principle of priority"[3]

Both Dr. Heikertinger's proposal of a principle of continuity and Dr. Smith's critique are of interest to taxonomists in zoology as well as in botany. The present remarks are intended to expand Dr. Smith's review and to carry on the arguments against Heikertinger's proposal.

The goal which Dr. Heikertinger hopes to attain with his new proposal is very attractive. It is that *within 30 years every species of animal will have one single universal name in use.* This is the millennium in nomenclature, the goal of complete stability which has seemed so far away to most taxonomists. This goal is to be attained by discarding the principle of priority and substituting for it the so-called principle of continuity, that "the valid name of a genus or species is the one which the monographer finds in scientific usage, regardless of whether or not this is the earliest name."

This statement of principle immediately raises several questions which must be satisfactorily answered before the principle could be applied in actual practice. (1) Who is to be accorded the status of monographer with authority so much above the ordinary taxonomist? (2) If monographers disagree, which is to be accepted? (3) How can biological considerations be kept separate from nomenclatural ones, or, as Dr. Smith implies, are biological facts to fall at the monographer's whim along with the nomenclatural ones? (4) What will happen when the monographer bases his work on totally inadequate bibliographic or taxonomic research and makes an obvious and demonstrable error? (5) Would complete stability be reached even with the elimination of purely nomenclatural changes?

(1) Apparently the question of who is a monographer is not discussed by Heiker-

[1] Received May 14, 1948.

[2] HEIKERTINGER, FRANZ. *Das Nomenklaturproblem der Gegenwart. Zugleich ein Aufruf an alle Biologen.* Der Biologe, 1942: pp. 20–27.

[3] Direct quotations are from Dr. Smith's paper.

tinger. There is an implication that the monographer is someone special, readily distinguished from other workers. One group of "monographs" is mentioned that may give us a clue. Heikertinger states that zoological nomenclature was more or less established about 1850, because of the thorough monographs of that period. Since Heikertinger is an entomologist, these monographs were probably the large regional works such as the Naturgeschichte der Insecten Deutschlands, and the numerous large works of Erichson, Kraatz, Redtenbacher, Mulsant, and others.

These works were of restricted geographical scope, being in fact not true monographs at all but revisions of the species of one region. They doubtless helped to fix names in use locally for a few years, but their influence in the long run depended upon their accuracy in a broad sense. Stable nomenclature can not be based upon the names in use in one region, and it is to works of this sort, based on less than a world viewpoint, that we owe much of the confusion in names with which we now contend, because the names were thereby brought into common usage. A monograph of a genus or large group for the world will give the only sound results, both taxonomically and nomenclaturally. There have been works of this nature in many groups and at many times, but no decade produced enough to claim a stabilizing effect on all zoological nomenclature.

A publication that has the appearance of being a monographic study may fall far short of complete or adequate treatment. For example, a recent work on a world-wide genus of insects, purported to deal with all the known species. It gives keys and descriptions and distribution and was based on extensive material obtained from all over the world. The work thus purports to be a monograph of the genus. Yet on closer examination it appears that at least half of the specific names that had previously been used in the genus are not mentioned, and numerous cases of homonymy and objective synonymy are completely overlooked. Even if the zoological aspect of this study is thoroughly and competently treated, the nomenclatural treatment is so bad that the revision is nearly useless. It is even possible to find the genotype of one subgenus listed in a different subgenus! To accept this work as a monograph for the purpose of stabilizing names would mean throwing overboard not only the principle of priority but also the concept of genotypes as the anchor of generic names and the requirement of thoroughness and accuracy for general acceptance.

In short, the only way to define a monograph in the sense of Heikertinger would be to set up an authority to pass on each publication. Acceptance of any given work as a monograph on a certain group of animals would automatically set up a list of *nomina conservanda* for both genera and species in that group, except for changes required on taxonomic grounds, as will be discussed under question 5.

(2) Disagreement between monographers would be taken care of by the authority mentioned above. If the acceptance of one work as a "monograph" did not serve to discourage a later work on the same subject, the authority would have to pass on the later work when it appeared. Rejection of the later work would uphold the earlier one, but acceptance of the later one would perhaps reverse some nomenclatural as well as taxonomic decisions and cause name changes.

(3) Many apparently nomenclatural decisions are based at least in part on purely taxonomic considerations. Under the law of priority, the correct name for any species is the oldest nonpreoccupied name that has been applied to it, *assuming that unrecognized biological identity with another so-called species does not exist.* This assumption is seldom expressed but always exists. When it can be demonstrated that there *is* taxonomic identity, there is certain to be a change in the status of one of the names. This change is nomenclatural and is made because of the law of priority, and yet abolition of that law would not prevent the change because of the biological considerations.

Nomenclaturally we hold that each genus must have a type species. The actual identity of that species cannot be determined nomenclaturally, however, for it is

necessary to make at least a morphological study of the type specimens of that species to demonstrate its characteristics. Under a recent interpretation, apparently employed by the International Commission in Opinions 168, 169, 173, 175, 177, 179, and 181, it is even necessary to examine the specimens that were before the worker who subsequently selected the genotype. These are biological considerations, although the problem of genotype fixation is generally thought of as primarily nomenclatural.

It is simply impossible in many cases to separate taxonomic from nomenclatural considerations, and if nomenclatural problems are to be decided by the whim of a monographer, it is difficult to see how we can prevent confusion of the taxonomic facts. Dr. Smith interprets Heikertinger's position thus: "These zoological monographs, one is led to believe, should be preferred to the older often superficial works, even when the monographic concept of a species differs from the original concept, and even when this difference in concept is caused by the monographer's misinterpretation of an earlier writer's type specimen."

(4) Heikertinger appears to believe that a monographer will always be in a position to make a sound decision on which name is in current use. Yet very few studies take into account *all* the previous literature and *all* the previous specimens. In actuality our monographs vary from this down to mere compilations or condensations which critically evaluate none of the previous work.

Some decisions of some monographers would inevitably be demonstrated to be based on inadequate or erroneous data. To refuse to reverse such a decision would be ridiculous, yet the principle of continuity would require just that.

If writer Jones finds that *P. niger* is in use and is to be retained over the older *P. obscurus*, does this decision give permanence to *P. niger* even when it is pointed out that Jones failed to note that *niger* is a junior homonym? This is a strictly nomenclatural change, but if both *nigers* are in current use, continuity could save only one of them.

In view of the low quality of the bibliographic work of some monographers, it is likely that in some cases a later monographer would be able to prove that the first monographer failed to consider a large number of pertinent works which would tend to reverse his decision. One world authority on a family of insects is unable to keep track of even his own proposals. He has repeatedly used a name in one genus not twice but three times. In one case, discovering the homonymy of two of his names he renamed the younger. The new name was promptly recognized (by another worker) as a homonym of another of his and renamed. Several years later this writer *rediscovered* the original homonymy and again renamed it, using the same new name as before, now twice a homonym as well as a junior synonym. This same writer habitually pays no attention to genotypes. It is not difficult to believe that any nomenclatural decision he made in a monograph or elsewhere is at least likely to be seriously defective.

(5) It is a popular pastime among certain biologists to ridicule the taxonomists for the large number of name changes that are made, generally implying that it is because of religious fervor for certain Rules of Nomenclature that such changes are proposed. In this way nomenclature is often made to take the blame for all changes of name. There can be little doubt that this is a most misleading assumption. Many names are changed because of discovery of older synonyms or the recognition of forgotten homonyms of prior date. These are the only truly nomenclatural changes. But many more changes are made (at least in some groups of organisms) because of generic transfer, proposal of segregate genera, recognition of generic equivalence, and similar purely biological considerations. And many are made because of nomenclatural requirements growing out of zoological actions, such as renaming of concurrent homonyms produced by union of genera. In many groups it is not difficult to demonstrate that a substantial majority of changes of names over a period of years has been caused by the second and third means listed above, namely those involving zoo-

logical actions rather than exclusively nomenclatural ones. These changes can not be prevented at all by the "principle of continuity."

If Heikertinger's proposition were to be adopted and means provided to make it work, we would still have changes of names as long as students search out new facts of relationships of organisms. Some changes would be prevented, it is true, but these could probably be prevented more easily by other means.

The overemphasis on the need for complete stability of nomenclature is demonstrated by a quotation by Dr. Smith from a German botanist. This botanist contends that we fail in our responsibility to our studying youth by making them unlearn the names each semester to follow the latest changes. Dr. Smith replies that this exaggerated statement "scarcely causes us to shed a tear, since this same (student) is expected to discard preconceived notions of all other branches of biological science at the drop of a chromosome. Why is it that workers in other fields of biology expect absolute stability of systematics (that is, comparative morphology and its attendant nomenclatural expression), while they are willing to accept any degree of flux in the fields of genetics, physiology, cytology, and . . . sociology?"

The reason for this emphasis on stability is doubtless the desire of these scientists to have means of tying their experiments and theories definitely to specific kinds of organisms, in order to use them for synthesis and generalization. But this desire for fixed names is impossible of gratification under any system as yet dreamed of and should not be given consideration over the necessity of the science of taxonomy for growth and development itself. All means should be found to prevent unnecessary changes of names, but it is not to be expected that a rapidly growing science like taxonomy can be for long conducted with an unchanging set of tools. Nomenclature will become stable only when monographs of high quality have been produced, based on all possible material, bibliographic sources, and techniques, and even these cannot be expected to stand indefinitely against new information and conceptions.

Dr. Smith criticizes the attitude of certain non-taxonomists as follows: "Too many criticisms of the present Rules of Nomenclature are based upon the assumption that professional systematists are playing a malicious game which has no relation to the biological sciences. The authors of these criticisms tacitly assume that systematists already have reached all the conclusions necessary regarding the classification of plants and animals, and that only their innate perversity prevents them from publishing a final and immutable list of the "correct" names of all living things. Immutability is not to be found in science, least of all in a virile branch like systematics, which builds upon facts disclosed by many other disciplines, each of which in itself is vigorous and, as human endeavor goes, young."

Systematists can find many reasons for wanting to reduce to a minimum the changes of scientific names, but this does not mean that absolute stability is the principal goal of systematics or of nomenclature. The goal of systematics is to discover the relationships between organisms so they may be classified in a usable system. The goal of nomenclature is to provide a method of designating the organisms explicitly, with as much uniformity and permanence as the growth of the classification permits. Any proposal that rates stability ahead of the advancement of the science of systematics or the development of one of its myriad components is a backward step and one doomed to ultimate failure and discard.